Effects of transparency on customer-supplier contracting

Jody Grewal, Aditya Mohan and Gerardo Pérez-Cavazos*

October 31, 2019

Abstract

We exploit the 2017 introduction of Payment Practices Disclosure Regulation in the United Kingdom to examine the effects of mandating disclosure on customer-supplier payment practices. We find that large firms reduce their accounts payable by 12.7% while non-disclosing small and medium-sized enterprises (SMEs) reduce their accounts receivable by 11.3%. Cross-sectional tests indicate that higher expected reputational costs are an important channel driving the change in payment practices. Further, SMEs with stronger competitive positions and lower financial constraints are better able to capture the benefits. We also find evidence suggesting economic efficiency gains: SMEs reduced short-term debt by approximately 3.5% and were awarded an additional 4.5% in government contracts. Lastly, newly disclosed information shows that large firms accelerate payments and increase the fraction of invoices paid within agreed terms.

Keywords: *payment practices, disclosure regulation, trade credit, customer-supplier relations*

---

* Jody Grewal is an Assistant Professor at the University of Toronto. Aditya Mohan is a PhD Candidate at Harvard Business School. Gerardo Pérez-Cavazos is an Assistant Professor at Harvard Business School. We thank Hans Christensen, Jonas Heese, Paul Healy, Andreya Pérez-Silva, and seminar participants from the Accounting & Management Workshop at HBS for helpful comments, suggestions and feedback. Jody Grewal acknowledges financial support from the Office of the Vice-President, Research and Innovation at the University of Toronto.

1. Introduction

More than 80% of world trade relies on trade credit (World Trade Organization). While financing firm operations through suppliers is customary, inefficiencies can arise when buyers do not pay within agreed terms or when large creditworthy companies finance themselves through their less creditworthy, smaller suppliers.[1] These inefficiencies are especially challenging for small and medium-sized enterprises (SMEs) that have limited access to financing. In this paper, we study the effects of disclosure regulation on payment practices. Specifically, we exploit the introduction of the Payment Practices Disclosure Regulation (hereafter, PPDR) in the United Kingdom (U.K.) to examine whether requiring firms to disclose information about trade-credit payment practices influences when and how small suppliers are paid by their large customers.

Although securing timely payments from customers is a concern for firms of all sizes, prior research suggests that smaller companies are more likely to experience late payments, resulting in financial distress and foregone investment and growth (Murfin and Njoroge, 2015; J. N. Barrot, 2016; Fabbri and Klapper, 2016). SMEs face the challenge of doing business with large customers that have greater bargaining power to negotiate favorable prices and payment terms; such customers also have greater legal resources with which to defend themselves if an action for late payment is brought to court. To mitigate these challenges, the U.K. introduced PPDR in 2017. This regulation requires large U.K. firms to disclose detailed information about their payment practices relating to U.K.-based contracts. The required disclosures include statistics such as the average number of days a firm takes to pay its suppliers and the fraction of invoices that are not paid within agreed terms, as well as narrative information such as how the firm deals with disputes regarding payments. Firms must submit this information to the U.K. Government, which publishes it on a publicly available website.[2]

[1] In the Americas, the fraction of business-to-business (B2B) overdue invoices was greater than 48% in 2017 (Atradius, 2017).
[2] See: https://publish-payment-practices.service.gov.uk

The stated objective of PPDR was to help small U.K. suppliers obtain improved payment terms from large customers (Department for Business, Energy and Industrial Strategy, (DBEIS, 2017)). However, previous efforts in this area by U.K. regulators failed to achieve the desired results. Hence, ex-ante, it was unclear whether PPDR would be successful.[3] There are a few reasons why disclosure might not impact customer-supplier contracts. First, given that customers and suppliers typically play a repeated game where interactions occur frequently over time, suppliers likely have accurate expectations of the payment behavior of each of their customers, which they can incorporate into the transaction price. As a result, mandating that customers disclose payment terms may not affect negotiations between customers and suppliers. Second, disclosure might not lead to changes in payment terms because customers derive real benefits such as reduced financing needs (or even generating financing income) by delaying payments to their suppliers. These benefits might outweigh the costs associated with public disclosure of payment terms. Finally, the disclosure might also prove ineffective due to the preexistence of alternate sources of payment practices information like firm financial statements and credit agency reports.[4]

Alternatively, the new disclosure rule may affect the equilibrium via two different mechanisms. First, a particular supplier can use the newly disclosed data to learn about how their customers pay their *other* suppliers. Access to this information could improve suppliers' bargaining position.[5] For example, learning that a customer typically pays its suppliers in 25 days might lead a supplier that receives payments in 55 days to renegotiate and seek faster repayment. Second, after the passage of this rule, customers' payment terms become publicly available, and customers with poor payment practices could suffer from negative publicity and reputational damage. Customers may therefore alter payment practices in response to public pressure (or the threat of it). Regulators explicitly stated in the regulatory documents that they anticipated behavioral change from PPDR as a result of public pressure, usage of the reports by suppliers,

[3] In Section 2, we provide a brief overview of previous efforts by U.K. regulators to influence payment practices.
[4] Although information sources such as credit agency reports are costly to obtain, especially for SMEs, they might already provide some of the information PPDR intends to make public and hence dampen the effect of the regulation.
[5] Consistent with the model developed by Admati and Pfleiderer (2000), this is a likely reason why firms do not voluntarily disclose detailed payment practices.

and responsible companies leading the way and encouraging best-in-class payment practices (DBEIS, 2018). Ultimately, whether and how the disclosure of payment practices affects customer-supplier relations is an empirical question.

This setting also presents a number of desirable features from a research-design perspective. Firms started disclosing payment practices at different points in time throughout 2017, 2018, and 2019 according to their fiscal year-end month.[6] This staggered adoption of PPDR allows us to use (high-dimensional) time fixed effects, which alleviates concerns that our results are spuriously driven by concurrent but unrelated market-wide events, such as other economic, regulatory, or institutional changes (Leuz and Wysocki, 2016). In our primary specification, we estimate the effects of disclosures of payment practices by comparing changes in accounts payable between disclosing and non-disclosing firms in the U.K. The identifying variation in this generalized difference-in-differences (DID) design comes from firms that meet the arbitrary reporting thresholds stipulated by PPDR.[7] Moreover, smaller firms are not subject to the additional disclosure requirements but can benefit from the increased transparency of their larger customers. Therefore, we also compare changes in accounts receivable between SMEs (not subject to additional disclosure requirements) and large firms to examine whether SMEs – the intended beneficiaries of PPDR – have lower accounts-receivable balances following increased disclosure from large firms.

In our main analysis, we examine the effects of PPDR on firms that are subject to the new disclosure requirements (hereafter, large firms). In addition to the reasons cited above, another reason we may not be able to detect effects for large firms is that many of these firms have significant international operations

[6] The earliest mandated reporters were firms with an April year-end; the first payment-terms reports from these firms were due on November 30, 2017. Firms with a different year-end month were granted additional time. For example, the first reports for firms with a December 31, 2017 year-end were due on July 30, 2018. The full schedule of first-year reports is found on page 20 of the Guidance document: https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/649941/payment-practices-performance-reporting-requirements-oct-2017.pdf

[7] Firms had to meet two of the following three criteria in the most recent two fiscal years to be subject to the increased disclosure requirements: 1) sales greater than £36 million (USD $43m), 2) assets greater than £18 million (USD $21m), and 3) more than 250 employees.

and contracts with non-U.K. suppliers that fall outside the scope of the regulation.[8] Alternatively, given that nearly 84% of U.K. companies surveyed by Atradius in 2015 claim "uniform payment practices" across all their customers, there may be spillover effects from U.K.-based suppliers to non-U.K.-based suppliers, resulting in a detectable change in overall payment behaviors. The results of our analyses suggest that large firms *do not*, on average, decrease their accounts payable as a ratio of their revenues (*A/P*). However, we find that large firms with a significant proportion of U.K.-based revenues decreased *A/P* by 12.7% on average following PPDR. This finding suggests that following increased transparency of payment practices, large firms with significant operations in the U.K. reduced the time they took to pay their suppliers. We also examine whether SMEs benefitted from this indirect treatment. We document a statistically significant reduction in accounts receivable as a ratio of revenues (*A/R*) for SMEs. In addition, we find a reduction of 11.3% *A/R* for the subset of SMEs with a higher fraction of U.K. revenues. This finding is consistent with SMEs receiving faster repayment from customers following the regulation, resulting in lower *A/R*.[9] Additional tests suggest that the parallel trends assumption is valid and that alternative non-regulatory explanations for our results are unlikely (see Figure 1).

Next, we perform cross-sectional tests on large firms to analyze the mechanisms through which PPDR changed payment behaviors. First, information about payment practices revealed in the reports may exert pressure through public shaming and may discipline firms by increasing their expected reputational costs (e.g., Rauter 2017; Dyreng, Hoopes, and Langetieg 2018; Dyck and Zingales 2005). This societal and public pressure is particularly effective if end-consumers purchase directly from firms because they can punish firms for unfair payment practices by not purchasing their products. This pressure may also be more effective if the firm receives more media attention and coverage from the business press. To test this

[8] As described in section 2, the mandated disclosures relate to contracts with "a significant connection with the U.K.", which typically means that the contract will be performed in the U.K., or where both parties are established in the U.K. or carry out a relevant part of their business in the U.K.

[9] An alternative denominator for our *A/P* variable is cost of goods sold (COGS), which would help capture days payable. We scale accounts payable by revenues to be consistent across our A/R and A/P variables. However, in untabulated robustness tests we scale A/R by COGS and find similar results in terms of economic and statistical significance.

channel, we assess whether changes in payment behavior are concentrated in firms that sell their products to end-consumers (business-to-consumer firms or B2C) and firms that receive more media attention. Using retail firms as our proxy for B2C, we document that B2C firms with significant U.K. operations decreased *A/P* by an additional 14.6%. Moreover, large firms with high media attention, captured by the number of mentions that a firm receives in the business press, reduce their *A/P* significantly. This suggests that increase in reputational costs is an important mechanism through which PPDR generated changes in payment practices. Importantly, we do not detect behavioral changes in the period between the announcement of the regulation and payment reports being released, suggesting that firms underestimated the reputational and regulatory costs of PPDR.

We also conduct cross-sectional tests for SMEs to investigate which firms primarily benefit from PPDR. We document that SMEs with relatively stronger competitive positions experienced larger decreases in *A/R* and that the decline in *A/R* was accentuated for firms with lower cash constraints (or higher cash balances as a fraction of total assets). Although regulators intended for PPDR to benefit SMEs with poor negotiating positions and cash flow concerns (DBEIS, 2017), our results indicate that SMEs with stronger competitive positions and greater financial resources prior to PPDR were more successful in obtaining improved payment terms from their suppliers following PPDR. This suggests that a number of threats on the part of the supplier – for instance, the threat of walking away from the contractual relationship, the threat of pursuing legal action for late payment, or the threat of publicizing unfair treatment – were made increasingly credible by the transparency of payment records and the increased regulatory support for fair payment practices.[10]

An important motivation for the regulators' objective to improve payment practices for SMEs is economic efficiency. For one, late payment forces small companies to increase short-term borrowings to

[10] Anecdotal evidence suggests that such threats did, in fact, materialize. Since PPDR came into effect, the Office of the Small Business Commissioner (SBC) has seen an increase in the number of late payment cases brought to their attention from small companies who, in the past, were reluctant to challenge their large customers. Since PPDR, the SBC has mediated thousands of late-payment cases and has helped resolve payment disputes worth £400,000 with larger businesses (The Guardian, 2019).

finance working capital. In addition, delayed payment hinders growth, preventing SMEs from bidding on large contracts that require high levels of competitiveness and funding needs. We assess whether PPDR resulted in improvements in economic efficiency by examining changes to short-term debt and contracts awarded to SMEs. We document that following PPDR, SMEs experienced a decrease in short-term debt scaled by assets of 3.3% to 3.7% relative to large firms, with the majority of this decrease being due to lower short-term financing by SMEs rather than higher short-term financing by large firms. We also find that SMEs experienced an increase of 4.5% in the value of U.K. government contracts awarded relative to large firms. Again, our results suggest that the majority of this result is due to larger contracts being awarded to SMEs following PPDR, rather than changes to contracts awarded to large firms. Additional analyses suggest that this result is not likely being driven by an increase in the supply of contracts that SMEs can bid on in the post-PPDR period.

Finally, we utilize the information disclosed by large firms in their payment practices reports to provide a more nuanced understanding of how firms changed their payment behaviors following PPDR. Our analyses show that for each subsequent payment report released by a firm (there are at most four reports available per firm at the time of our analysis), firms reduced the fraction of invoices not paid within the contractually agreed time by 1.5%. This finding is economically significant, as the mean fraction of invoices not paid within the agreed terms is 31%. We also document a significant shift in payment periods. Our estimates suggest that the average firm with significant U.K. operations accelerated payment of 4% of their invoices to within 30 days, while reducing the fraction of invoices paid between 31-60 days and over 60 days by 2.2% and 1.7%, respectively.

Our study contributes to two strands of literature. The first examines the effects of disclosure regulation on firms' decision-making.[11] Extant research documents that mandated disclosure generates real effects or "changes in behavior in the real economy (e.g., investment, use of resources, consumption)" owing to the improved transparency that contracting stakeholders use to influence the reporting firm's

[11] Prior work studying the effects of disclosure regulation focuses on firm-level investment (Cheng et al., 2013; Rauter, 2017), internal capital allocation (Cho, 2015), mine safety (Christensen et al., 2017), and bank failures (Granja, 2018).

decisions (Leuz and Wysocki, 2016). Our study differs from prior work in two important ways. First, in our setting, suppliers already possess private information about the payment practices of their customers and rationally incorporate this information into the equilibrium price. Consequently, we study whether information about the *average* payment practices of a given customer – which allows suppliers to benchmark their terms relative to those of other suppliers – improves the ability to negotiate better terms. Second, unlike prior research, which mainly focuses on the effects for disclosing firms, our setting allows us to also examine the effects on the intended beneficiaries of the regulation.

We also contribute to the strand of research that studies customer-supplier payment practices. Payment practices, despite being a central element of customer-supplier relationships, have received little attention in prior literature as contract-level data is typically not available (Mian and Smith, 1992).[12] Existing research documents the adverse consequences of long payment terms, which include financial distress, liquidity risk and lower investment at the supplier level (Murfin and Njoroge, 2015; J.-N. Barrot, 2016; Costello, 2018); in contrast, accelerated payments increase suppliers' ability to hire new employees and increase existing workers' compensation (Barrot and Nanda 2016). Prior work also documents that large, important customers with strong bargaining power relative to their suppliers are more likely to extend the payment period and generate overdue payments (Klapper et al., 2012; Fabbri and Klapper, 2016). However, little is known about how suppliers can improve payment conditions. To our knowledge, we are the first to document that transparency on customer payment practices is an effective tool for reducing payment durations and improving payment terms.

The results of our paper also have important implications for policy makers around the world. Beyond the U.K., late payment practices make headlines in the European Union, South America, China, and the United States. In some instances, these practices have led to substantial research endeavors and interventions by governmental organizations. In 2006, for example, Chile limited the trade credit terms that

[12] Moreover, suppliers are an important source of financing for most firms, with accounts receivable representing over 20% of the total assets of the average firm in Compustat.

a large retailer could obtain from its small suppliers (Breza and Liberman, 2017). At the same time, disclosure regulation has increasingly become the "weapon of choice" for regulators whose objective is to allow market forces to change firm behavior by removing information asymmetry frictions. Although suppliers already have reasonable expectations for the payment terms of their customers based on past transactions, our study shows that disclosure can be an effective way to shift the balance of power between suppliers and customers via disciplining channels such as public pressure and information about payment practices, which suppliers can use to negotiate better terms.

The rest of the paper proceeds as follows. Section 2 describes the setting. Section 3 presents the data. Section 4 presents the main empirical analyses and results. Section 5 further examines the payment practices disclosure reports. Section 6 presents additional robustness tests. Section 7 concludes.

2. Setting

In December 2016, the U.K.'s Department for Business, Energy and Industrial Strategy (DBEIS) adopted regulations made under Section 3 of the Small Business, Enterprise and Employment Act 2015; these regulations introduce a duty on the U.K.'s largest companies and LLPs to report their payment practices, policies, and performance for financial years beginning on or after April 6, 2017. The mandated disclosure requirements affect companies that exceed at least two of the following three thresholds in the previous two financial years: 1) sales greater than £36 million, 2) assets in excess of £18 million, and 3) more than 250 employees. Covered firms are required to submit a report every six months within 30 days of their usual semiannual fiscal year end date describing payment terms related to contracts for goods, services, or intangible property that have a significant connection to the U.K.[13] Contracts for financial services are excluded from the reporting requirements, including insurance-related services and banking services.

[13] Whether a contract has a significant connection with the U.K. depends on the circumstances. However, examples would include a contract that will be performed in the U.K., or where one or both parties is established in the U.K. or conducts a significant part of their business in the U.K. (DBEIS, 2017).

The disclosure requirements can be broadly categorized into three groups: statistics, narrative, and check-the-box statements. The main statistics required to be disclosed are (1) the average number of days taken to make payments to suppliers, (2) the fraction of payments that were paid in 30 days or less, between 31 and 60 days, and in 61 days or longer, and (3) the percentage of payments that were not paid within the agreed terms. Firms are also required to provide narrative descriptions of standard payment terms (including contractual length of time for payment of invoices and maximum contractual payment period) and the process for resolving payment disputes. Last, the check-the-box statements include whether suppliers are offered e-invoicing and whether supply chain finance is available to suppliers.[14]

The regulation also dictates that submitted reports will be publicly accessible on a web-based service provided by the government. All qualifying entities within a business group are required to submit a report. In other words, each individual entity that meets the size thresholds is required to report, with no option for consolidated reporting. To ensure compliance, the Government considers the failure to publish a payment practices report within the specified filing period as a criminal offense by the business and every director of the company. Reports containing misleading, false, or deceptive information are also considered criminal offenses.[15]

PPDR came as a response to concerns about the financial burden faced by SMEs that are not paid on time. Using survey data, the DBEIS found that late payment is a concern for 55% of SMEs (DBEIS, 2018). Moreover, over half of SMEs that experience late payments wait a month or longer beyond the agreed terms to be paid, and nearly a quarter of U.K. businesses report that late payments are a threat to their survival. The regulatory documents also relayed findings from research conducted by the U.K. Federation of Small Businesses which found that 37% of small businesses face cash flow difficulties owing

[14] Supply chain finance refers to cases where the supplier receives the payment from a finance provider or other third party rather than from the qualifying company. If the supplier receives the full amount due, then the date on which the supplier received the payment can be reported as the date of payment.

[15] A research report issued by the Government to quantify the costs of the regulation estimated that the average cost at the individual company level is £9,895 for "initial one-off costs including familiarization with the proposed new requirements, information gathering, IT costs and changes to processes" and £4,071 for "ongoing annual costs including maintaining systems and processes and preparing, collating, approving and submitting reports twice yearly."

to late payment; 20% report that late payment has caused a profit slowdown; and an estimated tens of thousands of U.K. businesses are failing each year due to late payments (DBEIS, 2017).

PPDR is the "most stringent action taken by the U.K. to address the issue" (Alvarez and Marsal, 2017); it is not, however, the first effort to improve payment practices in the U.K. In 1998, the Late Payment of Commercial Debts (Interest) Act created a statutory framework for addressing late payment and providing suppliers the right to charge interest on late payment and reclaim administrative costs for pursuing late payment. In 2008, the Chartered Institute of Credit Management (on behalf of Government) established the Prompt Payment Code (the Code) to promote a culture of prompt payment. Signatories to the Code agreed to pay 95% of invoices within 60 days and work towards 30 days as normal practice; they also committed to other standards of good practice, such as not retroactively changing payment terms.[16] Although the Code demonstrated the intentions of its signatories, payment records were not reported.

The survey results summarized above suggest that these previous regulations were not particularly effective at improving payment terms for SMEs. Unequal bargaining power between SMEs and their large customers meant that SMEs were reluctant to use existing legislation to pursue late payments or to challenge the status of a signatory to the Code (DBEIS, 2017). Although the intent of PPDR is for transparency to yield public pressure and allow SMEs to improve their bargaining position (DBEIS, 2018), the muted effects of these prior regulatory efforts raises questions as to whether PPDR will result in improved payment terms for SMEs and increases the possibility that PPDR will not produce a change in payment practices for large, disclosing firms.[17]

[16] Over 2,000 organizations were signatories to the Code as of September 2018. The Code's signatories make a public commitment to pay on time and pay fairly. Signing the Code acts as a signal of quality in terms of payment practices for other businesses considering doing business with the signatory businesses and also provides a statement of good practice within the business community.

[17] Given the lack of proximity between the timing of these actions and the regulation we are studying (i.e., 1998 and 2008, whereas PPDR came into effect in 2017), we do not expect these actions to confound the effects of PPDR. Our parallel trends results and falsification tests in prior years further rule out the possibility of our results being confounded by these previous regulations.

3. Data

We obtain data on firms' financial characteristics from Bloomberg, as it provides us with the most comprehensive set of required data on both public and private firms incorporated in the U.K. To extract the data from Bloomberg, we take the following steps. First, we filter firms based on their country of incorporation and keep firms that (1) were in operation as of 2016 and (2) are incorporated in the United Kingdom.[18] At this stage, we keep both public and private firms. Second, we impose a data availability requirement for our key variables: accounts payable, accounts receivable, revenues and assets. In the steps where we run the difference-in-differences specifications, we only keep observations from firms where revenues and total assets are available for the years used to assign treatment and control status. Finally, we drop firms in financial services and utilities, as their operations are significantly different from other firms. Moreover, financial contracts were not covered under PPDR. The panel for our main analyses includes approximately 12,000 firm-year observations from 1,200 firms over the 2009 to 2018 period.

We also obtain data about payment practices from the reports mandated by PPDR. We download the payment reports from https://check-payment-practices.service.gov.uk/export and delete observations with a duplicate company number and filing date. Our tests are limited to examining how companies change their payment practices from one report to the next because, by construction, there are no reports during the period prior to PPDR. Consequently, we remove 1,034 reports from firms that have only filed one report as of June 2019. The final sample comprises over 13,000 observations from 5,812 unique firms.

Finally, we download contract data between U.K. government agencies and their suppliers from https://www.gov.uk/contracts-finder. Since 2015, U.K. government agencies must publish procurement opportunities and contracts awarded (over £10,000) on Contracts Finder, a website administered by the Government. We download contracts from 2015 to 2019, which provides us with over 68,000 contracts from 2,635 different U.K. government agencies and 5,825 unique U.K. suppliers.

[18] We impose these restrictions, as they match the requirements set by the PPDR of meeting the specified size thresholds for the previous two years.

4. Empirical Analysis and Results

4.1. Empirical Methodology

We examine how the disclosure requirements imposed by PPDR affected firms using a difference-in-differences methodology. We separately estimate the effects on the disclosing firms (i.e., large firms) and the intended beneficiaries of the regulation (i.e., SMEs). More specifically, we examine whether the PPDR led to changes in Accounts Payable for large firms and in Accounts Receivable for SMEs. The basic regression we use to estimate the effect on large firms is as follows:

$$A/P_{i,t} = \alpha_0 + \alpha_1 Large_{i,t} + \alpha_2 Post_{i,t} + \alpha_3 Large_{i,t} \times Post_{i,t} + \alpha_i + \alpha_t + \epsilon, (1)$$

where the dependent variable $A/P_{i,t}$ is accounts payable scaled by revenues for firm i in year t. The independent variable for the first difference, $Large_{i,t}$, takes the value of 1 for firms that meet the minimum size thresholds set by the regulation (i.e., firms with assets over £18 m and sales over £36 m in the previous two fiscal years) and 0 otherwise.[19] The second independent variable, $Post_{i,t}$, takes the value of 1 beginning with the disclosure of payment practices reports in November 2017 and 0 otherwise. It is important to note that the regulation affected firms in a staggered fashion depending on their fiscal year end, so $Post_{i,t}$ can take the value of 1 and 0 in the same month for two different firms. The variables α_i and α_t denote firm and year fixed effects, respectively. In all the regressions, we cluster standard errors at the firm level.

As described earlier, it is *ex-ante* difficult to predict the effect of PPDR on large firms. Many firms have significant operations outside of the U.K. and contracts with non-U.K. suppliers that fall outside of the disclosure requirements. As a result, we might not find an effect on the payment practices of those firms. However, it is possible that if firms improve payment practices with their U.K. suppliers, this could

[19] Regulation PPDR also includes a threshold in terms of number of employees. We ignore this threshold, as number of employees is largely unavailable for our sample. However, we conduct two (untabulated) tests to mitigate concerns relating to this research choice. First, we analyze the distribution of the number of employees for the firms for which these data are available. We find that in 2016, over 95% of the firms that met the assets and revenue thresholds also met the number-of-employees threshold. Second, we rerun our regression models using only treated firms that we could match to a firm in the payment practices report. We obtain similar results. Finally, note that even if we erroneously classify a subset of firms as small when they are in fact large, this would bias against finding an effect.

influence the payment practices for their other, non-U.K. suppliers (i.e., a spill-over effect).[20] A challenge in our setting is the inability to observe whether firms' accounts-payable balances are owed to their U.K. suppliers or to their non-U.K. suppliers. Therefore, we first estimate the effect of PPDR on the full set of firms that are mandated to disclose, without discerning whether more or less of their business is U.K.-based. Then, we re-estimate the effect on firms with significant U.K. operations to assess whether these firms have a stronger response to PPDR. In particular, we use geographic segment data from Worldscope to identify firms with significant operations in the U.K.[21] We create a firm-specific, time-invariant indicator, $High\ UK$, that takes the value of one if more than a third of the firm's revenues (averaged over the sample period) originated in the U.K., which is the threshold used by S&P's Global Market Intelligence and Geographic Segment Analysis products to classify high exposure to a certain region or country.[22] Our assumption is that firms with high U.K.-based revenues contract more with other U.K. firms and will therefore respond more strongly to PPDR. In our main analyses, we present the results of this test, specifically the interaction $Large \times Post$ for the subsample of $High\ UK$ firms, alongside the results for large firms.

To examine the effect of the regulation on SMEs, we estimate the following regression:

$$A/R_{i,t} = \alpha_0 + \alpha_1 SME_{i,t} + \alpha_2 Post_{i,t} + \alpha_3 SME_{i,t} \times Post_{i,t} + \alpha_i + \alpha_t + \epsilon, (2)$$

where the dependent variable $A/R_{i,t}$ is defined as accounts receivable scaled by total revenues. We set the value of $SME_{i,t}$ to 1 for firms that do not meet the size requirements in terms of assets or revenues set by the regulation and 0 otherwise. Although we anticipate that the majority of smaller U.K. firms will

[20] Nearly 84% of surveyed U.K. companies claim to have consistent payment practices across all customers – regardless of size or location – suggesting that spill-overs from improved payment practices for one set of customers are plausible (Atradius, 2015).

[21] Publicly-traded U.K. companies are required to apply IFRS (which includes the IFRS 8 "Operating Segments" standard). Private U.K. companies can adopt EU IFRS or UK GAAP, both of which also require segment reporting. Although firms may choose to disclose segment data based on products/services rather than geographic markets, close to 92% of our sample reports geographic segment data.

[22] See the product manuals, which can be accessed through the platforms (by subscription) on https://www.spglobal.com/marketintelligence/en/solutions/market-intelligence-platform. Our results are similar if we define High UK using different thresholds, such as more than 50% of sales originating from the U.K.

conduct their business primarily in the U.K., we also estimate equation (2) for the subset of firms with significant operations in the U.K., or $High\ UK$ firms.

4.2. Descriptive Statistics

Table 1 reports descriptive statistics for the observations in our sample. We winsorize all continuous variables at the 1 and 99 percentiles and calculate ratios using the winsorized variables. Panel A of Table 1 reports descriptive statistics for the full sample. The average firm in our sample has assets of £740 m and annual revenues of £621 m. These statistics are substantially different for the 50.04% (49.96%) of firms defined as large (SMEs) under the PPDR. Large firms (SMEs) have on average £1.4bn (£34 m) in assets and £1.2bn (£11 m) in revenues. In terms of trade credit, the average firm in our sample has £55 m in accounts payable and £58.7 m in accounts receivable. Large firms hold £105 m in accounts payable and £108m in accounts receivable. In contrast, SMEs hold £1.5 m in accounts payable and £1.9 m in accounts receivable.

The average firm in our sample has short-term debt (as a percentage of total assets) of 7%, with large firms owing 5% of total assets and SMEs owing 9%. The mean (median) percentage of U.K.-originated revenues for the full sample is 34% (33%). As expected, large firms have more of their revenues originating outside of the U.K. than do SMEs; large firms have median U.K.-originated sales of 20%, while this figure is 46% for SMEs.

4.3. Main Results

We report the main results of equations (1) and (2) in Table 2. The coefficient on $Large \times Post$ in column 1 estimates the effect of the regulation on A/P (accounts payable scaled by total assets) for the full set of large firms in our sample that are subject to the new disclosure requirements. After the increased transparency of payment practices, we do not find a statistically significant effect on large firms' accounts payable relative to firms that are not mandated to disclose. This result indicates that our full sample of large

firms is insufficiently exposed to the U.K., or that the effects of PPDR on payment behaviors with U.K.-suppliers did not spill-over to non-U.K. suppliers.

In column 2 of Table 2, we estimate the effect of the regulation on the subsample of large firms with significant U.K. operations. Consistent with our conjecture, the coefficient on $Large \times Post$ for *HighUK* firms is negative and significant (coef. = -0.127, std. error = 0.057). The coefficient estimate suggests that A/P of large firms whose operations are focused on the U.K. decreased by 12.7% following increased transparency of payment practices. After the introduction of the regulation, large firms with significant U.K. operations reduced the time they took to pay their suppliers, resulting in a lower accounts-payable balance as a ratio of revenues.

Although PPDR does not require SMEs to increase transparency or change their payment practices, they are the intended beneficiaries of the regulation. Therefore, it is important to understand whether SMEs indeed benefitted from this indirect treatment. Columns 3 and 4 of Table 2 report the effect of PPDR on A/R (accounts receivable scaled by revenues) for SMEs. In column 3, the coefficient estimate on $SME \times Post$ suggests that following PPDR, SMEs experienced a statistically significant reduction in A/R of 19.9% relative to large firms that were mandated to report their payment practices. In column 4, we estimate the model on the subsample of firms with significant U.K. operations and find a statistically significant reduction in A/R of 11.3%. These results are consistent with SMEs receiving faster payments from their customers following the increase in transparency of payment practices by large firms.

4.4. Announcement Effects

Our previous analyses examine the effects on payment practices *after* the disclosure of payment reports. We also examine whether firms changed their behavior following the passage of the regulation but before the new rules were implemented (i.e., *after* the regulation was announced but *before* any report was disclosed). We perform this analysis to help shed light on the mechanism(s) behind our main results. If one of the mechanisms through which firms change their behavior is the threat of reputational damage due to

poor payment practices, firms may preemptively respond to the implementation of PPDR and adjust payment practices prior to the release of payment reports. In contrast, if firms underestimate the level of attention and awareness that will be paid to the payment practices reports and anticipate that PPDR will be yet another weak attempt by regulators to improve payment behaviors, they will not react before the regulation's implementation.[23]

We investigate whether there is a regulation announcement effect by estimating equations (1) and (2) excluding firm-year observations after 2017 (i.e., when the regulation came into effect), applying the regulatory asset and revenue thresholds to firms in 2015 to identify the covered firms, and substituting the variable $Post$ with $Post\_Announce$, which takes the value of 1 beginning in March 2015 (the month when the initial announcement of the regulation was made), and 0 otherwise. Table 3 reports the results. The economically and statistically insignificant coefficient estimates for $Large \times Post$ in columns 1 and 2 indicate that *A/P* did not change for large firms after the announcement of the PPDR or for large firms with high U.K. operations. Similarly, the coefficients reported in columns 3 and 4 indicate that there was no significant decrease in *A/R* for small firms in the full sample or in the subsample of *HighUK* firms.

These results suggest that firms likely underestimated the attention that would be paid to payment practices reports, resulting in no detectable behavioral changes in the period following the regulation's announcement but before the regulation actually came into effect. Our results also point to the importance of the new disclosures, which allowed the public, customers, regulators and suppliers – armed with new information – to pressure firms to improve their behavior.[24]

[23] This expectation by firms would be rational given that previous endeavors by the U.K. Government (or its affiliates) to improve payment practices failed to produce the desired effects (see section 2 for more background). In addition, our conversations with U.K. executives suggest that firms did not anticipate significant attention to be paid to PPDR or the released reports.

[24] There is anecdotal evidence to support this. For instance, in April 2019, the U.K. Government announced that companies reporting poor payment practices risk exclusion from bidding on public contracts worth £5m or more per annum (Financial Times, 2019). Moreover, in July 2019, the U.K.'s Chartered Institute of Credit Management used the payment report data to suspend 18 companies from the Prompt Payment Code (Wall Street Journal, 2019). Although these actions occur after our sample period ends, it is reasonable to assume that they resulted from public pressure, negative attention, and increased awareness of poor payment practices following the release of the reports.

4.5. Cross-Sectional Analyses

Our main results indicate that PPDR led to a reduction in *A/P* and *A/R* for large and small firms, respectively. In this section, we describe the implementation and results of cross-sectional analyses. For large firms, our cross-sectional tests aim to uncover the mechanisms and channels through which PPDR generates behavioral changes relating to payment practices, and for small firms, our cross-sectional tests explore where the benefits of the regulation are concentrated.

4.5.1. Cross-sectional analyses for large firms

Disclosing firms may change their behavior to avoid a backlash from customers and the public against the firm or its products (e.g., Graham et al. 2013, Dyreng et al. 2015). Although we do not find that firms preemptively react to the threat of shaming and reputational damage by changing their behaviors *prior* to the release of the payment practices reports, firms may have underestimated the public's response to PPDR. Therefore, if payment disclosures discipline firms through increased susceptibility to reputational damage, the observed payment behaviors should be stronger for firms with higher expected reputational costs. To test this channel, we examine whether the decrease in *A/P* for large firms is accentuated for firms that sell their products to end-customers (i.e., business-to-consumer firms, or B2C) and for firms that receive more media attention. We expect reputational costs to be higher if end-consumers purchase from firms directly because they can punish firms by not purchasing their products. Moreover, we expect higher reputational costs for firms that receive more media attention because of greater societal and public pressure resulting from this media coverage.

Our proxy for B2C is whether the firm is a retailer because retailers directly interact with a large number of final customers compared to firms that only transact with other businesses (i.e., business-to-business firms, or B2B). Therefore, to the extent that consumer pressure increases firms' expected

reputational costs, we expect retailers to reduce their *A/P* more than B2B firms.[25] We create an indicator variable, *Retail*, equal to 1 if the firm is in the retail, internet/e-commerce, auto, hotel, airline or casino sectors, and 0 otherwise. We estimate a variation of equation (1) where the variable of interest is $Large \times Post \times Retail$, which estimates the effect of PPDR on *A/P f*or affected (i.e., large) U.K. firms that are retailers. The results are presented in Columns 1 and 2 of Table 4. In column 1, which uses our full sample, we find an insignificant coefficient on the triple interaction term (coef. = 1.067, std. error = 1.982). In Column 2, which uses the subsample of *HighUK* firms, we report a negative and statistically significant coefficient (at the 5% level) on the triple interaction term (coef. = -0.146, std. error = 0.058).

To measure media attention, we count the annual number of unique, English newspaper mentions that each firm receives in the business press based on press coverage data collected from Factiva. We classify firms as *High Media* if their media coverage exceeds the median of all sample firms in the year. Similar to our retail tests, we estimate a variation of equation for large firms (1) using $Large \times Post \times High\ Media$ as our main independent variable. Columns 3 and 4 of Table 4 report the results. We find a negative and statistically significant coefficient on the triple interaction term for the full sample (coef. = -0.791, std. error = 0.102) and the *HighUK* sample (coef. = -0.536, std. error = 0.103).

Overall, the findings across our two measures suggest that increased expected reputational costs, measured by firms that sell to end-consumers and firms that receive high media attention, played an important role in leading large firms with significant U.K. operations to improve their payment practices in response to PPDR. As mentioned above, given that we do not document an effect in the announcement period prior to the passage of the regulation, our findings suggest that firms did respond to public attention and scrutiny to payment practices but underestimated the public's response to PPDR.

4.5.2. Cross-sectional analyses for SMEs

[25] Please note that we only perform this test on large firms. We do not expect any changes in A/R for small retailers, as they receive their payments directly from retail customers.

In our cross-sectional tests for small firms, we are interested in identifying where the benefits of the regulation accrue, in terms of faster collection of payment from customers. In contrast to large firms covered by PPDR, SMEs are not subject to disclosure requirements but receive new information from large firms about their on-average payment practices. We examine which firms are able to use this new information to renegotiate improved terms with their customers and whether this aligns with the regulators' intended beneficiaries.

First, we examine whether the effect of PPDR on SMEs depends on their competitive position. On the one hand, regulators refer to small suppliers with weak negotiating positions as PPDR's intended beneficiaries (DBEIS, 2018). Thus, it is possible that SMEs with weaker competitive positions will have greater regulatory support to challenge their large customers and obtain improved payment terms, which will allow them to capture greater benefits from the regulation in terms of a larger decline in *A/R*. Conversely, strong relative competitive positions may allow small suppliers to benefit more from PPDR. Under this alternative, once SMEs are armed with newly-available information from the payment practices reports, they can more effectively leverage their strong competitive positions to renegotiate favorable payment terms, while their peers in weaker competitive positions may still be unwilling to challenge their large customers.

We estimate a variation of equation (2) where the variable of interest is $SME \times Post \times Asset\ Intensity$, which estimates the effect of PPDR on *A/R* for small U.K. firms that have strong competitive positions, as measured by their asset intensity.[26] The results are presented in columns 1 and 2 of Table 5. In column 1, which uses our full sample, we show a negative and significant coefficient (at the 1% level) on the triple interaction term (coef. = -0.0011, std. error = 0.0001). In column 2, which uses the subsample of *HighUK* firms, we document a negative and statistically significant coefficient (at the 5% level) on the triple interaction term (coef. = -0.0002, std. error = 0.00008). This suggests that once SMEs

[26] We measure asset intensity as the ratio of assets to revenues in the year 2015. We choose 2015 as the benchmark year because the regulation was announced in 2015. Our results are qualitatively the same if we use the average of cash intensities over multiple years around the announcement of the regulation.

with relatively high negotiating power are equipped with information from the payment practices reports, they are able to obtain better payment terms.

The regulation was also intended to benefit cash-constrained firms that could become financially distressed from late payments. As the regulatory guidance states, "Late payment is a key issue for smaller businesses as it can adversely affect their cash flow and jeopardize their ability to trade. In the worst case, late payment can lead to insolvency" (DBEIS, 2017, p. 3). We examine whether, as the regulation intended, the benefits of PPDR accrued to cash-constrained firms. The results for cash-constrained firms are reported in columns 3 and 4 of Table 5. The variable of interest, $SME \times Post \times Cash\ Intensity$, estimates the effect of PPDR on *A/R* for small U.K. firms as a function of cash intensity, measured as the ratio of cash to total assets.[27] The coefficient on the triple interaction term is negative but insignificant for the full sample in column 3 (coef. = -0.472, std. error = 0.329) and is negative and statistically significant (at the 1% level) for the subsample of *High UK* firms in column 4 (coef. = -0.1232, std. error = 0.0334). This finding indicates that firms with *higher* cash intensity benefitted more from the regulation. Although this is not in-line with PPDR's objective, our interpretation is that SMEs with greater financial resources were able to obtain improved payment terms due to the threat of walking away from a large customer, the threat of pursuing legal action for late payment, or the threat of publicizing unfair payment terms. Although SMEs with reduced financial constraints could threaten to do all of these things *before* PPDR, the credibility of these threats increased following PPDR because SMEs could use information from the payment records in negative publicity efforts and in legal actions, in addition to having increased regulatory support for fair payment. Anecdotal evidence suggests that such threats did, in fact, materialize. For instance, following PPDR, the Office of the Small Business Commissioner (SBC) experienced an increase in the number of late payment cases brought to their attention from small companies who, in the past, were reluctant to

[27] As in the case of asset intensity, we calculate cash intensity as the ratio of cash balance to assets in the year 2015 – the year when the regulation was announced. Our results are qualitatively the same if we use the average of cash intensities over multiple years around the announcement of the regulation.

challenge their large customers (The Guardian, 2019).[28] In addition, the U.K. Chartered Institute of Credit Management suspended 18 companies from the Prompt Payment Code following complaints from their suppliers – and data from the payment reports to support their complaints – that they were not compliant with the Code (Wall Street Journal, 2019).

4.6. Effects on Economic Efficiency

An important motivation for improving payment practices is economic efficiency. In a research report related to PPDR, the U.K. Government stated that "…Late payment [has] significant consequences for companies and the economy. It directly creates administrative costs for creditors and cash flow issues, and can indirectly lead to income loss, hindered growth and inability to pay or hire employees." (DBEIS, 2018, p. 16). A key issue that arises when large firms delay payment to small suppliers, is that small suppliers are forced to issue short-term debt to finance working capital (DBEIS, 2018). Given that the operations of large, mature companies are generally more stable than those of small firms, large firms likely face a lower cost of capital and it may not be efficient for small firms to finance large firms. In addition, when small firms are not paid on time, their growth is hindered because they lack the resources to invest (Beck and Demirguc-Kunt 2006). This prevents small firms from bidding on larger contracts that could contribute to their growth, as they are unable to demonstrate a capacity to fulfill awarded contracts. In this section, we examine whether PPDR resulted in improvements in economic efficiency along two key dimensions: short-term debt and the value of contracts awarded to SMEs.

To estimate the effect of PPDR on short-term debt of SMEs, we estimate our difference-in-differences model using short-term debt scaled by assets, *STD*, as the dependent variable. Table 6 reports the results. Columns 1 and 4 presents the coefficient estimates for the full and the *High UK* samples, respectively. Consistent with a reduction in short-term debt, we find that SMEs experienced a statistically significant (at the 5% level) reduction in *STD* of 3.3% to 3.7% relative to large firms. To understand whether

[28] Since PPDR, the SBC has mediated thousands of late payment cases and has helped resolve payment disputes worth £400,000 with larger businesses (The Guardian, 2019).

this effect is driven by a reduction in *STD* for small firms, an increase in *STD* for large firms, or a combination of both, we also estimate single difference models for small and large firms separately. Columns 2 and 3 report the results for the full sample, while columns 5 and 6 report the results for the *High UK* sample. We do not find a statistically nor economically significant change in *STD* for large firms. However, we find that SMEs decreased *STD* by approximately 3.6%, indicating that the majority of the diff-in-diff estimate reported in columns 1 and 4 is due to lower short-term financing by SMEs.

To estimate the effect of PPDR on the value of contracts awarded to SMEs, we use publicly available contract data between U.K. Government agencies and their suppliers. Ideally, we would like to examine whether PPDR affects the contracts between U.K. suppliers and *all* of their customers, but this data is not available. Therefore, we utilize data on U.K. public sector contracts between January 1, 2015 and September 30, 2019, accessed from https://www.gov.uk/contracts-finder. Since 2015, U.K. government agencies must publish procurement opportunities and contracts awarded (over £10,000) on Contracts Finder, a website administered by the Government.

The U.K. Government contract data provides information on the agency that awarded the contract, the contract description, the contract value, the date that the contract was awarded, and the name and contact information of the supplier that was awarded the contract. In some cases, there is information on the estimated start and end dates of the contract. There is also a field indicating whether a SME was granted the contract. First, we examine whether, following PPDR, SMEs are awarded larger, or higher-value, contracts from U.K. Government agencies. Our conjecture is that, if PPDR results in improved payment practices, smaller suppliers will be more willing to bid-on, and be able to demonstrate their capacity to fulfill, larger contracts. With more on-time payments from their customers, SMEs can more readily invest in the tangible and intangible assets needed to deliver on, and show suitability for, larger contracts.

To estimate the effect of PPDR on the contracts awarded to SMEs, we estimate our difference-in-differences model using the natural logarithm of contract value (*ContractValue*) as the dependent variable. Panel B of Table 6 reports the results. Consistent with larger contracts being awarded to SMEs in the post-

PPDR period, we find that SMEs experienced a statistically significant (at the 1% level) increase in *ContractValue* of 4.5% relative to large firms. This result is robust to the inclusion of U.K. Government agency fixed effects, supplier fixed effects and year fixed effects. To understand whether this effect is driven by an increase in *ContractValue* for SMEs, a decrease in *ContractValue* for large firms, or a combination of both, we estimate single difference models for SMEs and large firms separately. We do not find a statistically nor economically significant change in *ContractValue* for large firms (Column 2). However, in Column 3, we find that SMEs increased *ContractValue* by approximately 5.9%, indicating that the majority of the diff-in-diff estimate reported in Column 1 is due to larger contracts being awarded to SMEs following PPDR.

Our analysis suggests that SMEs are awarded larger contracts following PPDR relative to large firms. However, this could be due to increased contract opportunities for SMEs in the post-PPDR period (i.e. a demand-side effect) rather than SMEs becoming more competitive for larger contracts in the post-PPDR period (i.e. a supply-side effect). To disentangle whether the effect is due to demand or supply shocks, we examine whether the U.K. Government increased contract opportunities for SMEs in the post-PPDR period. As our dependent variable, we use an indicator, *SME_Eligible*, that is equal to one if the U.K. Government agency awarding the contract will consider bids from SMEs, and 0 otherwise.

Column 4 in Panel B of Table 6 presents the results of estimating a single difference model for the sample of contracts that are SME-eligible. We do not find a statistically nor economically significant change in *SME_Eligible* in the post-period. This suggests that the increase in contract value awarded to SMEs was not driven by an increase in the Government's demand for SME suppliers. Nevertheless, we cannot rule-out the possibility that the U.K. Government favors smaller suppliers in the post-PPDR period. However, procurement rules in the U.K. have certain controls in place to promote fair procurement practices, such as pre-qualification questionnaires which require buyers to evaluate potential suppliers on particular criteria and demonstrate that the criteria have been met before accepting a bid.

Overall, the results in this section suggest that PPDR lead to increases in economic efficiency via reduced short-term debt for smaller firms and larger government contracts being awarded to smaller firms, the latter of which could potentially contribute to their future growth.

5. Analysis of Payment Practices Disclosures

Our analyses thus far have focused on financial statement-based data and contract-level data to analyze the effects of PPDR. In this section, we utilize firm disclosures provided in the newly-mandated payment practices reports to provide a more nuanced understanding of how firms change their payment practices following PPDR.

PPDR requires affected firms to disclose their payment practices on a semiannual basis, within 30 days of their usual semiannual year-end date. The earliest mandated reports were for firms with an April year-end date; their first semiannual reports were due by November 30, 2017 and became immediately available on the regulator's website.[29] Required disclosures include statistics, narrative information and check-the-box statements (see section 2 for more information). Given that we do not have pre-PPDR disclosures, our tests are limited to an examination of whether and how firms' payment practices change from one report to the next.

5.1. Summary Statistics

As of June 2019, there were 14,268 reports available. Panel A of Table 7 presents summary statistics for the relevant numerical disclosures provided in the full set of reports. On average, the payment time for an invoice is 37 days; 53.5% of invoices are paid within 30 days, 14.6% are paid between 31 and 60 days, and 31.8% are paid in more than 60 days. Interestingly, more than 30% of invoices are not paid within the agreed terms. The regulator requires that these calculations be made based on the number of invoices and not their nominal balance. In Panel B, we provide statistics for the reports from *High UK* firms to facilitate

[29] The first report for firms with a December 31, 2017 year-end was July 30, 2018. The schedule of first-year reports is on page 20 of the Guidance document: https://assets.publishing.service.gov.U.K./government/uploads/system/uploads/attachment_data/file/649941/payment-practices-performance-reporting-requirements-oct-2017.pdf)

the interpretation of our later results; there are no notable differences between these statistics for the full sample compared to the subsample.

5.2. Results

Our specification for the analysis of payment practices reports is as follows:

$$Y_i = \alpha_0 + \alpha_1 Report\ Number_i + \alpha_i + \epsilon, (3)$$

where the dependent variable Y_i represents one of the numerical variables constructed using the disclosure reports, $Report\ Number_i$ takes a value between 1 and 4, depending on whether the metrics refer to the first, second, third or fourth report, and α_i denotes firm fixed effects.[30] We cluster standard errors at the firm level. Notably, unlike the previous set of analyses using financial statement data, in this analysis, we cannot run a difference-in-differences specification because we do not have payment terms data for SMEs. Therefore, the interpretation of results here is different from the previous analysis, where we could speak to the causal effect of the regulation on the payment terms of disclosing firms. In this setting, we can only report the within-firm time trend in payment practices for disclosing firms.

We expect that it takes time for consumers, the public, regulators, and the media to interpret and analyze the disclosures and exert pressure on firms to change their behavior. Similarly, we anticipate that renegotiations between customers and suppliers are time-consuming. Consequently, our specification explicitly models the expectation that changes to payment practices can occur over time. Table 8 reports the results of estimating this model. Panel A reports results for the full sample of reports, and Panel B reports results for the subsample of reports from *High UK* firms. In column 1 of Panel A, the coefficient on $Report\ Number$ is -1.543 (significant at the 1% level), indicating that with each report, firms reduce the fraction of invoices not paid within the contractually agreed time by 1.5%. Given that the mean fraction of invoices not paid as agreed is approximately 31%, this reduction is economically significant and suggests

[30] As of August 2019, the maximum number of reports available per firm was 4.

that firms reduce this fraction to 26% (on average after three reports). Moreover, the coefficient in column 1 of Panel B for the subsample of *High UK* firms is 2.5 times larger (coef. = -3.95, std. error = 0.602), implying that firms reduce the fraction of invoices not paid within the agreed time from 31% to 18% after three reporting periods.

It is possible that the reason for the reduction in the fraction of invoices not paid in agreed terms is simply because they increased the length of the contractual period in which they could make a payment. In column 2, we explore whether this is the case. We find that the standard contractual payment period actually decreases in both the full and the *High UK* samples. In addition, the coefficient on $Report\ Number$ in column 3 shows that the average payment time is unchanged in the full sample but decreases by 3.7 days per report for the *High UK* firms. The dependent variables in columns 3 to 5 measure the fraction of invoices paid within 30 days, between 31 and 60 days, and over 60 days, respectively. In Panel A, the coefficients on $Report\ Number$ from these models reveal a shift in payment practices. In particular, with each report, firms accelerate payment of 0.8% of their invoices, so that they are within 30 days instead of between 31 and 60 days. We document similar but economically larger results in Panel B for the subset of *High UK* firms. Our estimates suggest that *High UK* firms accelerated payment of 4% of their invoices to within 30 days, while reducing the fraction of invoices paid between 31-60 days and over 60 days by 2.2% and 1.7%, respectively.

The results of this test are consistent with large firms gradually continuing to improve their behavior after the implementation of the regulation. However, it is important to note that we are unable to disentangle whether this gradual effect is due to more data becoming available over time or simply due to the passage of time since each firm released its first report. This difficulty arises because supplier-customer negotiations are unobservable and because negotiating better terms is likely a time-consuming process.

6. Robustness Tests

An alternative interpretation of our main findings is that the change in accounts payable and accounts receivable is driven by a general time trend unrelated to the disclosure of payment practices. Although this is unlikely, given the reasons motivating the introduction of this policy, we run three additional tests. Specifically, we examine the validity of our parallel trend assumption for the difference-in-differences analyses and conduct two falsification tests.

6.1. Parallel Trends

A concern with using a difference-in-differences methodology arises if the treatment and control group are subject to different trends prior to the treatment event. We examine the validity of the parallel trend assumption in our setting by estimating a variation of equation (1) that includes the interaction of $Large$ or SME with a year indicator variable for each of the years 2009 to 2015. As shown in Table 9, we do not find statistically significant differences between large and small firms in the pretreatment period in the full or the *High UK* samples. Figure 1 provides a graphical representation of these findings. If the effect were driven by the differences between large and small firms and not by the disclosure mandated by the regulation, then we would expect to see effects in the preperiods. Thus, the lack of statistically significant coefficients in Table 8 further rules out the explanation that our results are being driven by the differential trends in the *AP* and *AR* ratios for large and small firms, respectively, rather than the regulation.

6.2. Falsification Tests

Our first falsification test assumes that the regulation came into effect in April of each of the years from 2011 to 2015, instead of its actual implementation date in April 2017. In each of the placebo years, we use the asset and revenue cutoffs specified by the regulation to classify firms into treatment and control groups. To remove the actual impact of the regulation, we remove observations from the years 2017 and 2018, as the regulation had already taken effect in those years. We then estimate regression (1) and report the results in Panel A of Table 10. As shown in the table, we do not find an economically or statistically significant

effect in each of the placebo years. The results from this test rule out that the change in firms' payment behavior was due to an event prior to the actual implementation of the regulation.

Second, having established that the effects were due to an event in 2017, a remaining concern is whether there was a concurrent event unrelated to the disclosure requirements. To rule out this possibility, we exploit the arbitrary size thresholds set by the policy. If the PPDR is driving our results, we expect that if $Large$ and $SMEs$ are defined using other arbitrary thresholds, the effects will weaken or disappear.

To conduct this falsification test, we create placebo cutoff points. In columns 1 to 4 of Panel B, we remove firms in a £50 m asset-and-revenue band around the regulatory thresholds.[31] We then define a treatment effect around a placebo cutoff of £100 m above the actual regulatory cutoffs (i.e., £136 m for revenues and £118 m for assets). The results reported in the first four columns of Table 10 show no treatment effect around this placebo cutoff. In columns 5 to 8, we repeat the same exercise while changing the placebo cutoff to £200 m above the actual regulatory cutoffs. At this placebo cutoff, for "*large"* firms, we do find a statistically significant *increase* in *A/P*. This suggests, that these placebo large firms actually increase their A/P relative to the placebo small firms. Finally, in columns 9 to 12, we remove firms in a £100 m asset-and-revenue band around the regulatory thresholds and define the placebo cutoff as £200 m above the actual regulatory cutoffs. We do not find a treatment effect under this placebo cutoff. These tests help mitigate the concern that a different event in 2017 drives our results. In addition, the lack of a significant decrease in *A/R* for placebo *SME* firms in Table 10 also highlights that firms strategically comply with the regulation by paying off smaller suppliers and not the larger ones. Taken together, the results from the falsification tests further confirm that our results are driven by the PPDR.

7. Conclusions

[31] The regulatory cutoff is £18m for assets and £36m for revenue. Therefore, in this placebo test, all the true control firms are dropped. From the sample of true treated firms, we drop all firms with revenues below £86m and assets below £68m. In un-tabulated results we find that results are consistent if we drop only the true control firms

This paper examines whether transparency about payment practices affects supplier-customer contracts. We exploit the introduction of PPDR, a regulation in the U.K. requiring large firms to disclose several metrics detailing how they pay their suppliers. Using this setting, we show that large firms significantly reduce their accounts payable after the introduction of PPDR. In addition, we investigate whether the intended beneficiaries of the increased disclosure, SMEs, indeed benefit from the regulation. Consistent with the objective of the regulators, we find that PPDR led to a significant reduction of 11.3% in *A/R* for SMEs with large exposure to the U.K. We find that the effect on large firms is more pronounced for retailers and for firms with high media coverage, suggesting that higher expected reputational costs is an important mechanism driving the change in payment behavior of large firms. In addition, we show that the effects for SMEs are concentrated in firms with stronger competitive positions. We also document that PPDR lead to increases in economic efficiency via reduced short-term debt for SMEs, and larger government contracts being awarded to SMEs, potentially contributing to their future growth.

Our analyses of the newly-mandated disclosure reports indicate that changes in payment practices occur gradually over time. We find that with each additional report issued, the fraction of invoices not paid within the agreed terms decreases by 1.5%, an economically meaningful magnitude. In addition, we document a significant increase in the fraction of invoices paid within 30 days. Taken together, our evidence indicates that disclosure regulation can be an effective tool to improve payment terms for small suppliers.

Our study contributes to both the disclosure and trade credit literature. Prior work shows that trade credit is one of the main sources of financing for companies and is typically beset by inefficiencies. By studying the effects of disclosure on payment practices, we show that transparency in this area can significantly shift bargaining power across customers and suppliers. These findings have important implications for academics who want to better understand the benefits of disclosure for non-disclosing parties and for regulators around the world concerned with improving payment practices.

References

Admati, A. R. and Pfleiderer, P. (2000) 'Forcing Firms to Talk : Financial Disclosure Regulation and Externalities', *The Review of Financial Studies*, 13(3), pp. 479–519.

Atradius (2015) *Payment practices barometer 2015. Available at: https://group.atradius.com/publications/payment-practices-barometer-great-britain-2015.html*.

Atradius (2017) *Payment practices barometer 2017. Available at: https://atradius.us/reports/publicationspayment-practices-barometer-united-kingdom-2017.html*.

Barrot, J. N. (2016) 'Can Paying Firms Quicker Affect Aggregate Employment?', *Working Paper*.

Barrot, J. N. (2016) 'Trade credit and industry dynamics: Evidence from trucking firms', *Journal of Finance*, 71(5), pp. 1975–2016.

Barrot, J. N. and Nanda, R. (2016) 'The employment effects of faster payment: Evidence from the federal Quickpay reform', *Working Paper*.

Breza, E. and Liberman, A. (2017) 'Financial contracting and organizational form: Evidence from the regulation of trade credit', *Journal of Finance*, 72(1), pp. 291–324.

Cheng, M., Dhaliwal, D. and Zhang, Y. (2013) 'Does investment efficiency improve after the disclosure of material weaknesses in internal control over financial reporting?', *Journal of Accounting and Economics*, 56(1), pp. 1–18.

Cho, Y. J. (2015) 'Segment disclosure transparency and internal capital market efficiency : Evidence from SFAS no . 131', *Journal of Accounting Research*, 53(4), pp. 669–723.

Christensen, H. B., Floyd, E., Yao, L. and Maffett, M. (2017) 'The real effects of mandated information on social responsibility in financial reports : Evidence from mine-safety records', *Journal of Accounting and Economics*, 64, pp. 284–304.

Costello, A. M. (2018) 'Credit Market Disruptions and Liquidity Spillover Effects in the Supply Chain', *Working Paper*.

DBEIS (2017) *Duty to report on payment practices and performance: Guidance to reporting on payment practices and performance. Available at: https://www.gov.uk/government/publications/business-payment-practices-and-performance-reporting-requirements*.

DBEIS (2018) *Creating a responsible payment culture: A call for evidence in tackling late payment. Available at: https://www.gov.uk/government/consultations/creating-a-responsible-payment-culture-a-call-for-evidence-on-tackling-late-payment*.

Dyck, I. J. A. and Zingales, L. (2008) 'The corporate governance role of the media', *Journal of Finance*, 63(3), pp. 1093–1135.

Dyreng, S. D., Hoopes, J. L. and Langetieg, P. (2018) 'Strategic subsidiary disclosure', *Working Paper*.

Dyreng, S. D., Hoopes, J. L. and Wilde, J. H. (2015) 'Public pressure and corporate tax behavior', *Journal of Accounting Research*, 54(1), pp. 147–186.

Fabbri, D. and Klapper, L. F. (2016) 'Bargaining power and trade credit', *Journal of Corporate Finance*, 41, pp. 66–80.

Financial Times (2019) 'Available at: https://www.ft.com/content/2fc5a0ce-56d7-11e9-a3db-1fe89bedc16e'.

Granja, J. (2017) ‘Disclosure regulation in the commercial banking industry : Lessons from the national banking era’, *Journal of Accounting Research*, 56(1), pp. 173–216.

Klapper, L. F., Laeven, L. and Rajan, R. (2012) ‘Trade credit contracts’, *The Review of Financial Studies*, 25(3), pp. 838–867.

Leuz, C. and Wysocki, P. D. (2016) ‘The economics of disclosure and financial reporting regulation: Evidence and suggestions for future research’, *Journal of Accounting Research*, 54(2), pp. 525–622.

Mian, S. L. and Smith, C. W. (1992) ‘Accounts Receivable Management Policy : Theory and Evidence’, *Journal of Finance*, 47(1), pp. 169–200.

Murfin, J. and Njoroge, K. (2015) ‘The implicit costs of trade credit borrowing by large firms’, *Review of Financial Studies*, 28(1), pp. 112–145.

Rauter, T. (2017) ‘Disclosure regulation, corruption, and investment: Evidence from natural resource extraction’, *Working Paper*.

The Guardian (2019) ‘Available at: https://www.theguardian.com/work-smarter/2019/may/22/the-scourge-of-late-payments-the-13bn-problem-undermining-uk-smes’.

Wall Street Journal (2019) ‘Available at: https://www.wsj.com/articles/u-k-companies-penalized-for-failing-to-pay-suppliers-on-time-11563385671’.

Appendix A. Variable Definitions

The following variables are constructed using data from Bloomberg [B], Worldscope [WS], regulatory filings available as of August, 2019 [RF], Factiva [F] and Contract Finder [CF].

Variable	Definition
A/P	Accounts payable as a ratio of revenues.[a] [B]
A/R	Accounts receivables as a ratio of revenues.[a] [B]
STD	Short-term debt as ratio of assets.[a] [B]
Large	Indicator variable for large firms as determined by the size thresholds set by PPDR. [B]
Post	Indicator variable for time periods after PPDR comes into effect (November 2017).
SME	Indicator variable for small and medium-sized enterprises as determined by the size thresholds set by PPDR. [B]
HighMedia	Indicator variable equal to 1 if the firm's annual media coverage, captured by the number of unique English newspaper mentions in the business press, exceeds the median of all sample firms in the year. [F]
HighUK	Indicator variable for firms whose fraction of revenues originating in the U.K. is greater than 33%. [WS]
Post_Announce	Indicator variable for the time period after PPDR is announced (March 2015) till its implementation (October 2017).
Retail	Indicator variable equal to 1 if the firm is in the retail, internet/e-commerce, autos, hotels, airlines or casino sectors, and 0 otherwise. [B]
Asset Intensive	Assets as a ratio of revenue. [B]
Cash Intensity	Cash as a ratio of assets. [B]
Short Term Leverage	Short term debt as a ratio of assets. [B]
ContractValue	Natural logarithm of one plus the contract value awarded to a supplier from a U.K. Government agency. [CF]
SME_Eligible	Indicator variable for contracts that are eligible for bidding by SMEs. [CF]
InvoicesNotPaidinAgreedTerm	Percentage of invoices not paid within agreed terms. [RF]
Avg_Pmt	Average time taken to pay an invoice. [RF]
Frac_30	Percentage of invoices paid within 30 days. [RF]
31_Frac_60	Percentage of invoices paid in more than 30 days but less than 60 days. [RF]
Frac_60+	Percentage of invoices paid in more than 60 days. [RF]
ShortestOrOnlyStandardPaymentperiod	Number of days to make payments, as set out in the standard payment terms. [RF]
Report_Number	Count variable. Equals n for the n^{th} report filed by a firm. [RF]

[a] We winsorize accounts payable, accounts receivable, and revenues at the 1% and the 99% levels before calculating these ratios

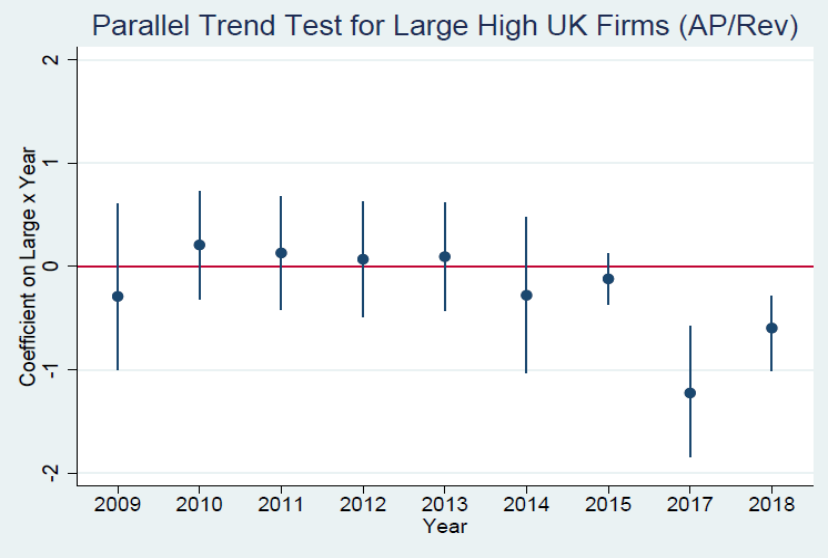
Figure 1. Parallel Trends

The following graphs reports the estimation results from a linear regressions of the following form:

$$Y_{i,t} = \alpha_0 + \alpha_1 Firm\_Type_{i,t} + \alpha_2 Post_{i,t} + \alpha_3 \Sigma Firm\_Type_{i,t} \times Year_t + \alpha_i + \alpha_t + \epsilon.$$

We analyze the differential time trends in the treatment and control groups by decomposing *Firm_Type x Post* term into mutually exclusive, *Firm_Type x Year* indicators for each of the years in the sample. The dependent variable is *A/P* for large firms and *A/R* for SMEs. Panels A and B report coefficients for the *Firm_Type x Year* indicators for large firms and SMEs with high exposure to the U.K, respectively. All models include firm and time fixed effects. All variables are defined in Appendix A, and the sample spans the period 2009-2018. Standard errors are clustered at the firm level and are reported below the coefficients. *, **, and *** indicate statistical significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Panel A. Large High UK Firms



Panel B. High UK SMEs

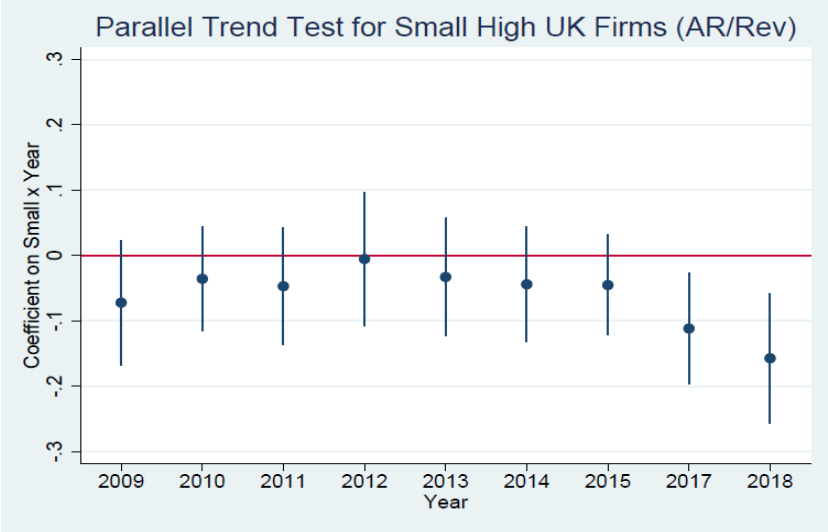


Table 1. Summary Statistics

This Table presents the summary statistics for the firm-year observations in our sample. Panel A presents statistics for the full sample of firms. Panel B presents statistics for the sample of firms whose operations are highly exposed to the U.K.

Panel A. Full Sample

Variable	Mean	Std. Dev.	25th	Median	75th	N	Mean (Large)	Mean(SME)
Payables	55,000,000	156,000,000	604,683	2,800,000	22,000,000	12,470	105,000,000	1,519,165
Receivables	58,700,000	148,000,000	1,000,000	4,400,000	32,000,000	11,861	108,000,000	1,911,466
Revenue	621,000,000	1,700,000,000	8,800,000	43,000,000	270,000,000	12,709	1,200,000,000	10,800,000
Assets	740,000,000	2,000,000,000	9,300,000	43,000,000	290,000,000	12,852	1,410,000,000	34,100,000
Short-term Debt	34,300,000	120,000,000	4,000	726,161	7,400,000	11,054	62,900,000	2,231,572
A/P	0.56	9.36	0.05	0.08	0.13	11,694	0.11	1.10
A/R	0.23	2.26	0.06	0.13	0.19	11,373	0.13	0.34
Short-term Debt/Asset	0.07	0.21	0.00	0.02	0.07	11,054	0.05	0.09
Large	0.50	0.50	0.00	1	1	14,190	1	0
HighUKOps	0.50	0.50	0.00	0	1	12,871	0.45	0.55
FracUKRev	0.34	0.27	0.15	0.33	0.61	12,871	0.31	0.37

Panel B. High UK Sample

Variable	Mean	Std.	25th	Median	75th	N	Mean (Large)	Mean (SME)
Payables	47,300,000	139,000,000	734,000	3,300,000	20,000,000	5,355	85,900,000	1,253,296
Receivables	52,200,000	134,000,000	1,200,000	4,500,000	28,000,000	5,293	93,700,000	1,796,646
Revenue	517,000,000	1,410,000,000	10,000,000	56,000,000	240,000,000	5,412	946,000,000	11,900,000
Assets	566,000,000	1,620,000,000	9,800,000	42,000,000	220,000,000	5,481	1,040,000,000	20,100,000
Short-term Debt	24,800,000	92,900,000	17,321	810,000	6,700,000	4,559	43,800,000	1,286,232
A/P	0.41	8.57	0.05	0.08	0.13	5,304	0.10	0.51
A/R	0.16	0.31	0.07	0.14	0.19	5,213	0.13	0.19
Short-term Debt/Asset	0.07	0.20	0.00	0.02	0.07	4,559	0.11	0.09
FracUKRev	0.55	0.23	0.35	0.53	0.74	6,436	0.42	0.57

Table 2. Difference-in-differences

This table reports the estimation results from a linear regressions of the following form:

$$Y_{i,t} = \alpha_0 + \alpha_1 Firm\_Type_{i,t} + \alpha_2 Post_{i,t} + \alpha_3 Firm\_Type_{i,t} \times Post_{i,t} + \alpha_i + \alpha_t + \epsilon.$$

The dependent variable is *A/P* when the analysis relates to large firms and *A/R* when the analysis relates to SMEs. Columns 1 and 2 report results for large firms for the full sample and the subsample of firms with high exposure to the U.K., respectively. Columns 3 and 4 report results for SMEs for the full sample and the subsample of firms with high exposure to the U.K., respectively. All models include firm and time fixed effects. All variables are defined in Appendix A, and the sample spans the period 2009-2018. Standard errors are clustered at the firm level and are reported below the coefficients. *, **, and *** indicate statistical significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Sample:	All Firms	High UK	All Firms	High UK
Dep. Variable:	A/P	A/P	A/R	A/R
	(1)	(2)	(3)	(4)
Large x Post	-0.964	-0.1273**		
	(1.677)	(0.0569)		
SME x Post			-0.199**	-0.1128***
			(0.0984)	(0.0205)
Post	0.322	-0.0988	0.104	0.0378
	(1.378)	(0.0607)	(0.0688)	(0.0412)
Constant	0.388***	0.188***	0.160***	0.165***
	(0.121)	(0.0177)	(0.0419)	(0.0120)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
Observations	11,694	5,304	11,373	5,264
R-squared	0.215	0.331	0.184	0.229

Table 3. Announcement Effect Analysis

This table reports the estimation results from a linear regressions of the following form:

$$Y_{i,t} = \alpha_0 + \alpha_1 Firm\_Type_{i,t} + \alpha_2 Post\_Announce_{i,t} + \alpha_3 Firm\_Type_{i,t} \times Post\_Announce_{i,t} + \alpha_i + \alpha_t + \epsilon.$$

The dependent variable is *A/P* when the analysis relates to large firms and *A/R* when the analysis relates to SMEs. Columns 1 and 2 report results for large firms for the full sample and the subsample of firms with high exposure to the U.K., respectively. Columns 3 and 4 report results for SMEs for the full sample and the subsample of firms with high exposure to the U.K., respectively. All models include firm and time fixed effects. All variables are defined in Appendix A, and the sample spans the period 2009 – October 2017. Standard errors are clustered at the firm level and are reported below the coefficients. *, **, and *** indicate statistical significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Sample:	All Firms	High UK	All Firms	High UK
Dep. Variable:	A/P	A/P	A/R	A/R
	(1)	(2)	(3)	(4)
Large x Post_ Announce	-0.360	-0.0041		
	(0.406)	(0.0223)		
SME x Post_ Announce			-0.0780	0.00051
			(0.128)	(0.0306)
Post_Announce	0.137	0.0076	0.0129	0.0071
	(0.244)	(0.0289)	(0.0260)	(0.0221)
Constant	0.311***	0.1651***	0.161***	0.165***
	(0.129)	(0.0127)	(0.0448)	(0.0127)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
Observations	10,527	4,655	10,210	4,655
R-squared	0.281	0.2407	0.199	0.24

Table 4. Cross-sectional Analysis: Large Firms

This table analyzes cross-sectional variation in the results related to large firms presented in Table 2. Columns 1 and 2 estimates the effects separately for retail firms as defined in Appendix A in the full and High UK samples respectively. Columns 3 and 4 estimate the model using cross-sectional variation in media and business press coverage in the full and High UK samples respectively. All models include firm and time fixed effects. All variables are defined in Appendix A, and the sample spans the period 2009-2018. Standard errors are clustered at the firm level and are reported below the coefficients. *, **, and *** indicate statistical significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Sample:	All Firms	High UK	All Firms	High UK
Dep. Variable:	A/P	A/P	A/P	A/P
Firm Characteristic:	Retail	Retail	High Media Attention	High Media Attention
	(1)	(2)	(3)	(4)
Large x Firm Char. x Post	1.0667	-0.1462**	-0.7910***	-0.5357***
	(1.982)	(0.0584)	(0.1019)	(0.1029)
Firm Char. x Post	-1.0747	0.1512***	0.7269***	0.4794***
	(1.982)	(0.0578)	(0.1010)	(0.1030)
Large x Post	-0.970	0.1272**	0.3595***	0.3009***
	(1.681)	(0.0574)	(0.0532)	(0.0681)
Post	0.328	-0.0993	-0.3414***	-0.2305***
	(1.386)	(0.0609)	(0.0551)	(0.0655)
Constant	0.388***	0.188***	0.1847***	0.1881***
	(0.121)	(0.0178)	(0.0135)	(0.0176)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
Observations	11,694	5,304	11,694	5,304
R-squared	0.215	0. 2583	0.611	0.4823

Table 5. Cross-sectional Analysis: Small and Medium-Sized Enterprises (SMEs)

This table analyzes cross-sectional variation in the results related to SMEs presented in Table 2. Columns 1 and 2 estimate the model using cross-sectional variation in asset intensity in the full and High UK samples respectively. Columns 3 and 4 estimate the model using cross-sectional variation in cash intensity in the full and High UK samples respectively. All models include firm and time fixed effects. All variables are defined in Appendix A, and the sample spans the period 2009-2018. Standard errors are clustered at the firm level and are reported below the coefficients. *, **, and *** indicate statistical significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Sample:	All Firms	High UK	All Firms	High UK
Dep. Variable:	A/R	A/R	A/R	A/R
Firm Characteristic:	Asset Intensity	Asset Intensity	Cash Intensity	Cash Intensity
	(1)	(2)	(3)	(4)
SME x Firm Char. x Post	-0.00109***	-0.0002**	-0.472	-0.1232***
	(0.000129)	(0.00008)	(0.329)	(0.03344)
Firm Char. x Post	0.00001	0.00002*	0.0221	0.03283
	(0.000)	(0.000)	(0.0270)	(0.0263)
SME x Post	-0.150	-0.09273	-0.0834	0.02382
	(0.0992)	(0.0969)	(0.0728)	(0.0772)
Post	0.0845	0.0224	0.103	0.03449**
	(0.0693)	(0.0376)	(0.0701)	(0.01478)
Constant	0.154***	0.168***	0.160***	0.165***
	(0.0425)	(0.0108)	(0.0425)	(0.0122)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
Observations	11,221	5,223	11,243	5,195
R-squared	0.182	0.38	0.184	0.227

Table 6. Economic Efficiency

The main results in this table report the estimation results from a linear regressions of the following form:

$$Y_{i,t} = \alpha_0 + \alpha_1 Firm\_Type_{i,t} + \alpha_2 Post_{i,t} + \alpha_3 Firm\_Type_{i,t} \times Post_{i,t} + \alpha_i + \alpha_t + \epsilon.$$

Panel A. Short-term debt

The dependent variable is STD for all firms. Columns 1 and 4 report results for large firms for the full sample and the subsample of firms with high exposure to the U.K., respectively. Columns 2 and 3 report results for change in STD between the pre and post periods for all large firms and SMEs, respectively. Columns 5 and 6 report results for change in STD between the pre and post periods for High UK large firms and SMEs, respectively. Columns 1 and 4 include firm and time fixed effects. Columns 2,3,5 and 6 include firm fixed effects. All variables are defined in Appendix A, and the sample spans the period 2009-2018. Standard errors are clustered at the firm level and are reported below the coefficients. *, **, and *** indicate statistical significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Sample:	All Firms	All Firms: Large	All Firms: SME	High UK	High UK: Large	High UK: SME
Dep. Variable:	STD	STD	STD	STD	STD	STD
	(1)	(2)	(3)	(4)	(5)	(6)
SME x Post	-0.033**			-0.037**		
	(0.016)			(0.016)		
Post	0.003	-0.005	-0.037**	0.013	0.000	-0.036**
	(0.008)	(0.003)	(0.016)	(0.010)	(0.005)	(0.015)
Constant	0.084***	0.052***	0.089***	0.088***	0.057***	0.094***
	(0.004)	(0.000)	(0.002)	(0.005)	(0.001)	(0.003)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	No	No	Yes	No	No
Observations	11,054	5,840	5,214	4,610	2,551	2,059
R-squared	0.419	0.566	0.395	0.439	0.588	0.309

Panel B. Contracts Awarded

The dependent variable in Columns 1-3 is *ContractValue* for all firms. The dependent variable in Column 4 is *SME_Eligible*. Columns 2 and 3 report results for change in *ContractValue* between the pre and post periods for large firms and SMEs, respectively. Columns 1 and 4 include firm and time fixed effects. Columns 2 and 3 include firm fixed effects. All variables are defined in Appendix A, and the sample spans the period 2015-2019. Standard errors are clustered at the firm level and are reported below the coefficients. *, **, and *** indicate statistical significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Sample:	All Firms	Large	SME	All Firms
Dep. Variable:	ContractValue	ContractValue	ContractValue	SME_Eligible
	(1)	(2)	(3)	(4)
SME x Post	0.0452***			
	(0.0118)			
Post	-0.2344	0.0047	0.0593**	0.0222
	(0.1755)	(0.1900)	(0.0253)	(0.0333)
Constant	0.2934***	0.2372***	0.4923***	0.6909***
	(0.0001)	(0.0001)	(0.0043)	(0.0293)
Supplier Fixed Effects	Yes	Yes	Yes	Yes
Gov't Agency Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No	Yes
Observations	68,920	45,934	22,986	4,610
R-squared	0.857	0.776	0.767	0.8117

Table 7. Summary Statistics - Payment Practices Reports

This table reports summary statistics on the information reported on the payment practices regulatory reports available on https://publish-payment-practices.service.gov.uk website as of August, 2019. Panel A provides summary statistics on the full sample. Panel B provides summary statistics on the subsample of firms whose operations are highly exposed to the U.K. All variables are defined in Appendix A.

Panel A. Full Regulatory Sample

	Mean	St Dev.	25th	Median	75th	N
Avg. Payment Time (days)	37.42	25.13	25	35	46	14,251
Invoices < 30days (%)	53.58	28.29	29	55	78	14,251
Invoices > 60days (%)	31.81	20.63	15	30	46	14,251
31days < Invoices < 60days (%)	14.60	17.00	3	8	19	14,251
Invoices Not Paid in Agreed Terms (%)	30.75	24.74	10	25	46	14,268
Standard Payment period (days)	21.62	24.56	1	28	30	14,268

Panel B. High UK Regulatory Sample

	Mean	St Dev.	25th	Median	75th	N
Avg. Payment Time (days)	40.42	27.6	28	37	49	1,479
Invoices < 30days (%)	50.56	26.5	29	50	72	1,479
Invoices > 60days (%)	32.67	18.5	19	32	46	1,479
31days < Invoices < 60days (%)	16.76	18.3	4	10	23	1,479
Invoices Not Paid in Agreed Terms (%)	31.21	24.4	12	26	46	1,479
Standard Payment period (days)	22.23	21	1	28	30	1,479

Table 8. Regulatory Results

This table reports the estimation results from a linear regressions of the following form:

$$Y_i = \alpha_0 + \alpha_1 Report\ Number_i + \alpha_i + \epsilon.$$

Panel A reports the results from the full sample and Panel B reports the results for the sample of firms whose operations are highly exposed to the U.K. The dependent variables are *InvoicesNotPaidinAgreedTerm,Shorterst_Std_Period , Avg_Pmt, Frac_30, 31_Frac_60, and Frac_60+*, in column 1 to 6 respectively. The main explanatory in all models is *Report_Number*. All variables are defined in Appendix A. All models include firm fixed effects. Standard errors are clustered at the firm level and are reported below the coefficients. *, **, and *** indicate statistical significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Panel A. Full Regulatory Sample

Dep. Variables:	InvoicesNotPaidinAgreedTerm	Shorterst_Std_Period	Avg_Pmt	Frac_30	31_Frac_60	Frac_60+
	(1)	(2)	(3)	(4)	(5)	(6)
Report_number	-1.544***	-0.264**	0.00437	0.808***	-0.797***	-0.00567
	(0.199)	(0.124)	(0.137)	(0.169)	(0.147)	(0.110)
Constant	33.59***	21.92***	37.17***	52.27***	33.11***	14.61***
	(0.334)	(0.209)	(0.230)	(0.283)	(0.248)	(0.185)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,066	13,066	13,057	13,057	13,057	13,057
R-squared	0.911	0.951	0.927	0.950	0.925	0.941

Panel B. High UK Regulatory Sample

Dep. Variables:	InvoicesNotPaidinAgreedTerm	Shorterst_Std_Period	Avg_Pmt	Frac_30	31_Frac_60	Frac_60+
	(1)	(2)	(3)	(4)	(5)	(6)
Report_number	-3.958***	-0.0280*	-3.733***	3.954***	-2.228***	-1.716***
	(0.6022)	(0.0154)	(0.4976)	(0.5379)	(0.4644)	(0.3119)
Constant	37.898***	22.281***	46.724***	43.873***	36.451***	19.651***
	(1.017)	(0.5231)	(0.8404)	(0.9087)	(0.784)	(0.5267)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,479	1,479	1,479	1,479	1,479	1,479
R-squared	0.9061	0.928	0.944	0.942	0.906	0.954

Table 9. Parallel Trend Analysis

This table reports the estimation results from a linear regressions of the following form:

$$Y_{i,t} = \alpha_0 + \alpha_1 Firm\_Type_{i,t} + \alpha_2 Post_{i,t} + \alpha_3 \Sigma Firm\_Type_{i,t} \times Year_t + \alpha_i + \alpha_t + \epsilon.$$

We analyze the differential time trends in the treatment and control groups by decomposing *Firm_Type x Post* term into mutually exclusive, *Firm_Type x Year* indicators for each of the years in the sample. The dependent variable is *A/P* when the analysis relates to large firms and *A/R* when the analysis relates to SMEs. Columns 1 and 2 report results for large firms for the full sample and the subsample of firms with high exposure to the U.K., respectively. Columns 3 and 4 report results for SMEs for the full sample and the subsample of firms with high exposure to the U.K., respectively. All models include firm and time fixed effects. All variables are defined in Appendix A, and the sample spans the period 2009-2018. Standard errors are clustered at the firm level and are reported below the coefficients. *, **, and *** indicate statistical significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Sample:	All firms	High U.K.	All firms	High U.K.
Firm type:	Large	Large	SME	SME
Dep. Variable:	A/P	A/P	A/R	A/R
	(1)	(2)	(3)	(4)
Post	-0.204	-1.163	0.014	-0.015
	(0.577)	(1.027)	(0.040)	(0.013)
Firm type x 2009	0.728	-0.290	-0.196	-0.072
	(0.586)	(0.545)	(0.158)	(0.074)
Firm type x 2010	0.783	0.208	-0.082	-0.036
	(0.726)	(0.266)	(0.175)	(0.062)
Firm type x 2011	0.542	0.130	-0.127	-0.047
	(0.563)	(0.278)	(0.160)	(0.070)
Firm type x 2012	0.968*	0.069	0.0127	-0.005
	(0.571)	(0.282)	(0.171)	(0.080)
Firm type x 2013	-0.0407	0.094	0.108	-0.033
	(0.788)	(0.254)	(0.177)	(0.071)
Firm type x 2014	0.383	-0.279	0.372	-0.044
	(0.508)	(0.383)	(0.443)	(0.069)
Firm type x 2015	0.475	-0.120	-0.042	-0.045
	(0.469)	(0.1258)	(0.114)	(0.060)
Firm type x 2017	-0.0809	-1.225**	-0.126	-0.011
	(0.810)	(0.531)	(0.123)	(0.066)
Firm type x 2018	-1.006	-0.597**	-0.234	-0.157**
	(2.466)	(0.286)	(0.149)	(0.078)
Constant	-0.0115	0.522	0.246***	0.197***
	(0.419)	(0.500)	(0.0619)	(0.033)
Firm Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
Observations	11,694	5,304	11,373	5,264
R-squared	0.216	0.159	0.185	0.229

Table 10. Falsification Tests

Panel A. Placebo Years

In the models below we report the results of placebo versions of our main analysis. We create treatment and control groups in each of the placebo years by applying regulatory asset and revenue cutoffs in those years. We limit the post period in each of the models to the year subsequent to the placebo treatment year to mimic our main analysis. The dependent variable is *A/P* when the analysis relates to large firms and *A/R* when the analysis relates to SMEs. Columns 1, 3, 5, 7 and 9 report results for large firms for the subsample of firms with high exposure to the U.K. Columns 2, 4, 6, 8 and 10 report results for SMEs for the subsample of firms with high exposure to the U.K. All models include firm and time fixed effects. All variables are defined in Appendix A, and the sample spans the period 2009-2018. Standard errors are clustered at the firm level and are reported below the coefficients. *, **, and *** indicate statistical significance at the two-tailed 10%, 5%, and 1% levels, respectively.

	Apr-11		Apr-12		Apr-13		Apr-14		Apr-15	
Variables	A/P	A/R	A/P	A/R	A/P	A/R	A/P	A/R	A/P	A/R
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Large x Post	0.041		-0.022		-0.006		-0.003		0.086	
	(0.034)		(0.046)		(0.041)		(0.148)		(0.249)	
SME x Post		-0.0006		0.015		-0.014		-0.031		0.023
		(0.019)		(0.021)		(0.028)		(0.021)		(0.058)
Post	-0.104*	-0.001	0.060	0.005	-0.016	-0.001	0.100	0.0004	-0.004	-0.021
	(0.056)	(0.023)	(0.089)	(0.021)	(0.042)	(0.015)	(0.307)	(0.001)	(0.195)	(0.026)
Constant	0.320***	0.161***	0.311***	0.161***	0.307***	0.164***	0.340***	0.167***	0.369***	0.169***
	(0.1262)	(0.001)	(0.122)	(0.011)	(0.119)	(0.011)	(0.117)	(0.015)	(0.157)	(0.016)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,978	1,962	2,522	2,502	3,086	3,058	3,683	3,644	4,252	4,211
R-squared	0.507	0.483	0.508	0.457	0.506	0.395	0.520	0.418	0.342	0.253

Panel B. Placebo Cut-offs:

In the models below we present results of placebo versions of our main analysis. To remove the actual treatment effect we remove firms in a revenue and asset band around the actual regulatory cutoffs. The band is £50 M and £100 M in columns (1) through (8) and columns (9) through (12), respectively. We create treatment and control groups by applying placebo asset and revenue cutoffs of £100 M and £200 M above the actual regulatory cutoffs in columns (1) through (4) and columns (5) through (12) respectively. The dependent variable is *A/P* for large firms and *A/R* for SMEs. Columns 1, 3, 5, 7, 9 and 11 report results for firms in the full sample. Columns 2, 4, 6, 8, 10 and 12 report results for firms with high exposure to the U.K. All models include firm and time fixed effects. All variables are defined in Appendix A, and the sample spans the period 2009-2018. Standard errors are clustered at the firm level and are reported below the coefficients. *, **, and *** indicate statistical significance at the two-tailed 10%, 5%, and 1% levels, respectively.

Deletion Cutoff:	Revenue: 86 m; Assets: 68 m				Revenue: 86 m; Assets: 68 m				Revenue: 136 m; Assets: 118 m			
Treatment Cutoff:	Revenues: 136 m; Assets: 118 m				Revenues: 236 m; Assets: 218 m				Revenues: 236 m; Assets: 218 m			
Sample:	All	High UK	All	High UK	All	High UK	All	High UK	All	High UK	All	High UK
Dep. Variable:	A/P	A/P	A/R	A/R	A/P	A/P	A/R	A/R	A/P	A/P	A/R	A/R
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Large x Post	0.024*	-0.009			0.019*	0.021**			0.010	0.032		
	(0.013)	(0.008)			(0.008)	(0.010)			(0.007)	(0.021)		
SME x Post			0.011	0.002			-0.002	-0.0017			-0.0142	-0.0089
			(0.009)	(0.008)			(0.007)	(0.007)			(0.011)	(0.008)
Post	-0.014	0.007	0.009	0.006	-0.007	-0.021	0.011	0.0035	-0.009	-0.033	0.004	0.0068
	(0.020)	(0.010)	(0.011)	(0.008)	(0.017)	(0.018)	(0.011)	(0.008)	(0.007)	(0.025)	(0.005)	(0.010)
Constant	0.105***	0.149***	0.148***	0.148***	0.105***	0.102***	0.148***	0.148***	0.087***	0.102***	0.132***	0.149***
	(0.003)	(0.030)	(0.011)	(0.023)	(0.003)	(0.005)	(0.011)	(0.023)	(0.003)	(0.005)	(0.013)	-0.030
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,970	1,706	4,923	2,202	4,970	2,197	4,923	2,202	3,999	2,197	3,964	1,706
R-squared	0.571	0.332	0.814	0.298	0.871	0.392	0.814	0.379	0.652	0.392	0.495	0.332